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Account by M. Leverrier of his Planetary Researches.

M. Le Verrier has communicated to the Academy of Sciences at Paris the following general account of his researches on the theories of the eight principal Planets: *Mercury*, *Venus*, the *Earth*, *Mars*, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*. *Comptes Rendus*, t. 79, No. 25 (21 December, 1874):—

The theory of *Neptune*, which I have the honour to present this day to the Academy, completes the *ensemble* of the fundamental theories of the planetary system, the first piece of which goes back to the 16th September, 1839, five and thirty years ago.

The numerous developments added from year to year are all of them mentioned in the *Comptes Rendus*. Some of them appear by their title only, and as they are dispersed in a large number of volumes, the Academy will, I hope, permit me, now that I arrive at the termination of this long discussion, to present a precise, but brief, *résumé* of them.

In 1849, already engaged for ten years in the work, and being in a position of better appreciating its difficulties, I presented the essential conditions in terms in which I have nothing to change.

None of the Tables, we said, intended to represent the movements of the planets accord rigorously with the observations. The most precise of them, those of *Mercury* and the *Earth*, leave much to be desired. I do not here speak of those irregular deviations which the uncertainty inseparable from all physical measurement introduces of necessity between observation and calculation; but of the systematic errors, the variation of which follows a determinate law, the real existence and regularity of which come out from the entirety of the observations of different observatories, and for which the theory alone is responsible.

These uncertainties deserve to fix our attention: doubtless they are inconsiderable, but, on the other hand, they exist everywhere, and their smallness does not authorise us to neglect them.

It would assuredly signify but little in itself that our astronomical tables made an error of a half-second in the time of a meridian passage, if the importance of this error did not consist in its degree of certainty rather than in its magnitude. Every deviation indicates an unknown cause, and may be the source of a discovery. If these deviations are to increase considerably with the time, we might, it is true, wait for their complete development to read with more certainty in their progressive march the cause which produces them: but first, we should leave to posterity the care of perfecting science, and the advantage of learning new truths: and next, certain foreign actions may manifest themselves by effects which are always little sensible;

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and if we should disdain these effects, the cause on which they depend would remain for ever unknown.

The theory of the movement of a planet rests on hypotheses the consequences of which were not on many points deduced with sufficient rigour; and on this account it was impossible to decide whether the disagreements found between observation and calculation depended merely upon analytical errors, or whether they were in part due to the imperfection of our knowledge of celestial physics.

It was therefore necessary to take up the mechanical theories of the movements of the planets, and to scrutinise them in their ultimate consequences, before it was possible to realise a decisive comparison with observations. This is what has been done.

We remark that the general developments have been the object of five Memoirs, presented and published in 1840, 1843, 1849, and 1855.

The formulæ relative to the secular inequalities have been treated in particular in the Memoirs of 1840 and 1841.

The same subject has been resumed in a more general and more complete manner in the work communicated to the Academy under the date 11 November, 1872, on the four great planets: *Jupiter*, *Saturn*, *Uranus*, and *Neptune*.

The theory of *Mercury*, presented in 1843, but afterwards completely remodelled, was definitely completed only in 1859.

The theory of *Venus* was given in 1861.

That of the Sun (the *Earth*) in 1853 and 1858.

That of *Mars* in 1861.

The theory of *Jupiter* in 1872 and 1873.

That of *Saturn* in 1872 and 1873.

The theory of *Uranus*, given in 1846, and connected with the discovery of *Neptune*, has been the object of a new work presented the 15th November last (1874).

Finally, the last theory, that of *Neptune*, is this day offered to the Academy.

The theories of *Jupiter*, *Saturn*, *Uranus*, and *Neptune* have the character that they are developed in functions of indeterminates, in such wise that their employment may be prolonged for an unlimited time.

The theories once established, it was necessary to compare them with the long and precious series of meridian observations imagined by Roemer, instituted for the first time at Greenwich, in the month of September, 1750, by the illustrious observer Bradley, and continued thence up to the present time in the great Observatories. But as the positions of the planets are referred to the fixed stars, it will be understood that it was also necessary to make sure of the fixed stars themselves, relatively to each other, and with respect to the equinox and the ecliptic. This necessity arises particularly in regard to the Right Ascensions, on which chiefly depends the knowledge of the movements of the

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planets. The work was effected in the Memoir of the 5th April, 1852, on the totality of the observations of Bradley. This was a delicate subject, for the question was to review Bessel's work, contained in his treatise entitled *Fundamenta Astronomiæ*. We had to propose various corrections to the positions of the fundamental stars, and the verification of the exactness of these corrections was proposed as a Prize question in Germany. The result confirmed all my determinations. Consequently, they have served to establish with security the positions of the comparison-stars during the one hundred and twenty years of observations which had to be considered.

The comparison of the movements of *Mercury* with the theory obtained in 1843 did not at first present a satisfactory result. The transits of *Mercury* afforded data of very great precision, and which could not be completely satisfied.

This first result naturally filled us with inquietude: had we not allowed some error in the theory to escape us? New researches in which everything was taken up by different methods ended only in the conclusion that the theory was correct, but that it did not agree with the observations. Long years passed, and it was only in 1859 that we succeeded in unravelling the cause of the anomalies arrived at. We recognised that they were all included under a simple law, and that it was sufficient to increase the motion of perihelion by thirty-one seconds in the century, to bring everything into order.

The displacement of the perihelion thus acquires in the planetary theories an exceptional importance. It is the surest index, when it has to be increased, of the existence of cosmic matter as yet unknown, and circulating, like the other bodies, round the Sun.

The consequence is very clear. There exists in the neighbourhood of *Mercury*, doubtless between the planet and the Sun, a matter as yet unknown. Does it consist in one or more small planets, or in more minute asteroids, or even in cosmic dust? The theory tells us nothing on this point. On numerous occasions trustworthy observers have declared that they have witnessed the passage of a small planet over the Sun; but nothing has been established on the subject ['on n'est parvenu à rien coordonner à ce sujet'].

We cannot, however, doubt the exactness of the conclusion. We shall, in fact, see the like analysis applied to the discussion of the observations of *Mars* conduct to an analogous consequence, and that this consequence is thus completely verified.

Bessel said of the theory of the Sun that it had not made the progress which might have been expected from the great number and the goodness of the observations. This appreciation long troubled our mind, too confident as we were in this pretended precision of the observations. After having reviewed, discussed anew the observations of the Sun made since the epoch of Bradley at Greenwich, at Paris, at Königsberg, to the number of

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9,000, our conclusion was quite different: viz., that the observations of the Sun leave much to be desired, by reason of systematic errors affecting them, and that there exists no discordance between theory and observation which cannot be attributed to the errors of this last.

Nevertheless, the discussion of the observations of the Sun led us at once to an important result connected with the great question which agitates at this moment the scientific world: a result which surprised us ourselves, so great was the false confidence inspired by the determination of the parallax of the Sun deduced by the Director of the Berlin Observatory from the transits of *Venus* in 1761 and 1769. I arrived at the conclusion, that the parallax of the Sun, then estimated at $8''\cdot57$, should be increased by the twenty-fifth part of its value.

Soon afterwards the comparison of the theory of *Venus* with the observations led to the same result, the necessity of augmenting by $\frac{1}{25}$ the parallax of the Sun.

Again, the theory of *Mars* led in its turn to an equally precise conclusion. It was established that it was impossible to satisfy the totality of the observations of *Mars* except by increasing the motion of its perihelion by about one-eighth.

This was the reproduction of the same fact as for *Mercury*, and the consequence to be derived was the same; viz. that the planet *Mars* must be subjected to the action of a quantity of matter as yet neglected, and which had to be estimated at about one-eighth of the mass of the *Earth*.

But then two hypotheses were possible, as we explained at the meeting of the 3rd June, 1861: either the hitherto neglected matter resided in the totality of the ring of the small planets, or else it had to be added to the *Earth* itself. In the second case, and as a consequence, the parallax of the Sun had to be increased by about one twenty-fourth part of its received value,—that is to say, we are led to the result already obtained from the theories of the Sun and *Venus*.

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Jupiter and *Saturn* have given rise to a theoretical work, the extent of which has been considerable, on account of the very large mutual perturbations of the two planets. The comparison of the theory of *Jupiter* with the observations has presented, after suitable modifications of the elements, a complete accordance. Thus the tables of *Jupiter* have been adopted by the Direction of the *Nautical Almanac* to serve in that important work. I owe to our colleague, Mr. Hind, superintendent of the *Nautical Almanac*, the satisfaction of having thus seen the adoption by the astronomical world of my Tables of *Mercury*, the Sun, *Venus*, *Mars*, and *Jupiter*, as they have successively appeared.

The Tables of *Saturn* are now constructed, and their comparison with the observations is almost completed.

The theories of *Uranus* and *Neptune* are also terminated, and there remains only to compare them with the observations.